SCIENCE

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MSS. intended for publication and books, etc., intended for review should be sent to Professor J. McKeen Cattell, Garrison-on-Hudson, N. Y.

THE OBJECT OF ASTRONOMICAL AND MATHEMATICAL RESEARCH[1]

DURING the first years in the life of the American Association for the Advancement of Science it was customary for the members to meet in much broader groups than they now do. As the membership grew and as the number of papers increased, it became necessary to divide the association into smaller and smaller groups. Section A as we now know it was organized in 1882. It was a happy circumstance that the plan adopted in that year did not separate the astronomer from the mathematician. For a time this section played a very important part in the history of American science; the meetings were well attended and both the mathematician and the astronomer contributed numerous and weighty papers. In more recent years our section has lost something of its former influence. The establishment, about twenty-five years ago, of what is now the American Mathematical Society did much to draw away the interest of mathematicians; and even of astronomers, for in the records of that society we find a goodly number of purely astronomical papers, and two of the earliest presidents were astronomers. Fifteen years ago what is now the American Astronomical Society was formed, and this has still further increased the separation between the two sciences. It seems a great pity that the two should so seldom find themselves together in the same room. The astronomer, in common with the physicist, the chemist and

[1] Address of the vice-president and chairman of Section A, Astronomy and Mathematics, American Association for the Advancement of Science, Philadelphia, December, 1914.

others, greatly needs the help that the mathematician can give. On the other hand, I believe that the mathematician has something to learn from the astronomer with regard to the point of view from which he pursues his researches. The difference in this respect between the two is becoming greater and greater. In choosing a subject for an address this afternoon, I thought it best to take advantage of one of the rare opportunities that an astronomer as such gains audience with the mathematicians, and to dwell upon this difference of view-point, with the hope of aiding in bringing together those who have meat and can not eat, and those who would eat but want it. Any such attempt, however ineffective by itself and however feeble in itself, is well worth while.

This difference in view-point is nothing more than a recurrence of the struggle that occurs in every kind of human activity between the essentials of a subject and the technique of that subject. It is a remarkable fact that the outcome of this struggle is not always in favor of the former, but that mere technique is sometimes able to gain permanent mastery and to submerge completely the objects for which it was created. The best illustration of this is to be found in the painter's art. We know that there was a time when painting was regarded as a mode of expression through which lessons might be taught and learned, or through which at least the world might be amused. But for many a long day painters have refused to take this view of their art. They hold in frank contempt a picture that tells a story, and their standards of what constitutes a great picture are unintelligible to any one who is not himself a painter. You will remember the picture by Whistler, at the Metropolitan Museum of Art in New York, called "A Woman in White." Although executed in oils, it is wholly in black and white. We are told that it was painted to show that certain effects could be produced in oils without the use of color. Here then is a painting that artists deem a great one, although to the general public it has no subject at all and conveys next to nothing. The majority of modern paintings belong to the same class and it has gotten to be well understood that artists are to paint only for other artists. In any definition of a great painting, skill and technique are indispensable, but a man is at once called a Philistine if he asks that artists use their talents for some other purpose than merely to record and exhibit personal achievement.

Painting and poetry are arts that in their essentials are much the same, their chief difference being one of tools. But while the painter has glorified his tools more and more, the poet has kept his head, and has not forgotten what tools are for. I suppose it would be possible to construct a poem without using any other vowels than *o* and *u*. If so we should have the literary counterpart of Whistler's "Woman in White." Of course such an effort would not be regarded seriously for a moment, nor should we tolerate in literature any mere exhibition of technique. Yet technique is quite as indispensable here as in painting, and great facility is as rare in the one art as in the other.

Astronomy and mathematics have their technique and are having their struggle with it. A century ago Gauss, a great mathematician and a great astronomer, speaking for his times as much as for himself, announced as his motto, "Pauca sed matura," and adopted as his crest a tree laden with fruit, few in number but remarkable for their perfection. Such sentiments as these and the feeling that lay behind them have undoubtedly done more to hinder the progress of science than to ad-

vance it. If there is any question as to what Gauss meant, we have only to turn to his biography to find the answer. He did not care to touch in print any subject that he felt he could not exhaust; merely to contribute to it seemed to him like plucking unripe fruit. Thus his published work, extensive though it is, represents only a part, and it may be only a small part, of the unremitting labor of this wonderfully fertile brain. We know, for example, that Gauss had developed the principles of the method of least-squares while he was still in his teens, but it was not until fourteen years later that he ventured into print on this subject. He would doubtless have wished to delay even longer had not Legendre in the meantime unearthed and published the same principles. We can make a good guess at the reasons for Gauss's delay. The method of least-squares is founded upon an assumption which can be put in various forms, but which always remains an assumption. Gauss would doubtless have wished to prove this assumption from fundamental principles or at least to have given it a more axiomatic dress; but this neither he nor any one that has come after him has succeeded in doing. An even better illustration of the former attitude of men of science in the matter of their obligations to science, is afforded by Gauss's part in the history of non-Euclidean geometry. In a letter to a friend he states that he had occupied himself extensively with Euclid's axiom concerning parallels and goes on to outline very briefly some of the results he had obtained. This letter contains all that is known of these researches. A few years after it was written Lobatchewski published the little book in which he proves that the parallel axiom is no axiom at all, but a pure assumption, and shows that another kind of geometry is imaginable in which the opposite assumption is made. In view of this work, it would have been necessary for Gauss to revise what he had already done before publishing it. He preferred, however, to suppress it altogether, and when after his death his scientific effects were overhauled, no trace of this subject was found among his papers.

It will be understood that it is not Gauss that I am presuming to criticize, but rather the times in which he lived. That was an age when it was taken for granted that a man should think of his scientific reputation as coming first, and when the form in which he gave his researches to the world was considered as important as their content. In more recent times the man of science has taken a new view of his calling and of his duties, and it is largely because of this new policy that progress has been so rapid in some directions. In astronomy, for example, the great strides that have been made in the present generation can be attributed to two things; first, there is the unprecedented concentration of effort. Great telescopes have been erected and great observatories have been built for the purpose of solving single problems or a single group of closely related problems. If these problems should remain unsolved in our time the work will be carried forward by a succeeding generation and perhaps completed many years after those who initiated it have passed away. Cooperation is another powerful implement that time has placed in the hands of the astronomer, more precious to him than any telescope or any observatory can be. Thanks to it, no pressing problem appears at present above our horizon that is too great for him to attack. If you will examine the working programs of our astronomical institutions, you will find that much the greater half of what they are doing is being carried out with direct ref-

ence to the needs and the activities of other institutions. Cooperation often makes severe demands upon the individual; it means that he must be willing to use his mental and his material equipment in furthering an impersonal plan; it means that he must sometimes subordinate his own judgment to that of others; it means that he must sometimes use methods that he would to like to modify in some particular if he were working alone.

I believe that it is true that the astronomer has broken more completely with ancient tradition than has the mathematician. Many of the latter are still inclined to take what may be called the artistic view of their work; they refuse to admit that mathematics is a means to some other end, and they frankly assert (half in jest and half in earnest) that their science need have no reference to material things. A few years ago a prominent mathematician, speaking I think from the very chair that I am vacating to-day, quoted with sympathy the sentiment that mathematics is born and nourished out of the play instinct of mankind. It is difficult for me to see the difference between this view and the view that a chess player takes of his game. In the one we may start if we like with a set of axioms and an arbitrary set of postulates without inquiring whether they apply to the world around us, and we may then amuse ourselves by tracing the consequences. The chess-player does this very thing: he sets out with a set of axioms that he calls rules and a set of postulates that he calls openings, and after the expenditure of much thought and ingenuity he is able to trace the consequences.

It is understood, I hope, that I have been speaking in averages. By no means all astronomers have gotten rid of the artistic notion in their work, and by no means all mathematicians have severed their connection with the real world by applying the square-root of minus unity. But there is no denying that the idea of cooperation in a broad sense has not yet taken a strong hold in mathematics. Whether as great advantage would flow from cooperation between one mathematician and another, as is the case in astronomy, it is not for me to say. But when we come to speak of cooperation between mathematics and the other sciences, the benefits that would follow are difficult to overestimate. Let me spend a few minutes in pointing out how greatly the help of the mathematician is needed in a single astronomical subject, namely, that which concerns spectroscopic binaries. If in these remarks I emphasize individual stars, Algol for example, you will understand that these are types of a large class, and that the problems they present are of cosmical importance.

The first star to be recognized as variable in its light was probably Algol. The Arabs seem to have made this discovery, for it is difficult to account otherwise for the very apt name they gave the star, Algol or El Ghoul, the changing spirit or demon. The same discovery was independently made by others, among them Goodricke of England in 1782, when he was eighteen years of age. Goodricke continued to observe the star until he had determined the period and the nature of the light changes, and he advanced what we now know to be the true explanation of its changing light, namely that Algol is periodically eclipsed by a darker companion of nearly the same size as itself. This conjecture was a very bold one in that day, for we must remember that binary stars were then unknown. A great many double stars had been detected, but it was supposed that these were the result of perspective and chance. It was about this

time that Michell showed that on the doctrine of probabilities double stars were too numerous to be fortuitous groupings in all cases, so that binary stars were in a sense discovered by a mathematician and not by an astronomer. Twenty years later Herschel proved at the telescope that some double stars are real binaries, and that they revolve around each other by reason of their mutual attractions.

In 1880 Pickering showed that Algol's changes in light conform well with the eclipse explanation, and he suggested that the matter might be settled by the spectroscope. He argued that the orbital velocity of Algol due to the attraction of the dark companion should be considerable, and should change its sign according as the observations are made before or after the time of minimum light. The spectroscope was not quite ready at that time to handle problems of such delicacy, but a few years later Vogel succeeded in greatly increasing its accuracy for the determinations of velocities, by substituting the photographic plate for the human eye. Algol was among the first stars to be tested by Vogel, and his observations indicate precisely such velocities as the eclipse explanation implies. This explanation has been accepted without reserve since that time, and has been extended to all the numerous variables of the same kind that have in the mean time been discovered.

It was early noticed by Argelander and others that the period of Algol, the time between two successive light minima, is not constant. Attempts were made to represent these inequalities by formulæ involving the second and higher powers of the time, but the star refused to conform to such equations. In 1888 Chandler examined this question with great thoroughness; he showed that by the introduction of *periodic* terms all the observations up to that time could be well represented. The most important of these terms has a coefficient of 173 minutes and a period between 130 and 140 years. To account for this Chandler supposed that the system contains a third body, and that Algol and its eclipsing companion revolve around the common center of gravity of all three bodies in this long period. The dimensions of this orbit were supposed to be such that the light equation in it for an observer on the earth would be 173 minutes, and thus the eclipses would be advanced or delayed by this amount, according as they occur on the nearer or the farther side of this vast orbit. Chandler was quick to see that this explanation entails irregularities in the proper motion of Algol, and that these might be large enough to be unearthed from meridian observations. An examination of all the material of this kind then available convinced him that such an effect is really present, the coefficient of the oscillation coming out 1.″3, and its period 131 years. This result was apparently confirmed in a general way by Searle at Harvard Observatory, making use of additional observations secured for this express purpose. Baushinger, however, after applying to the catalogue positions the best available systematic corrections, concludes that there is no evidence whatever of a periodic term in Algol's proper motion. In the following year, Boss overhauled the same observations once more and decided that the probabilities were in favor of the presence of a term with a period of 131 years, but with a coefficient much smaller than that found by Chandler, 0.″5 against 1.″3. In later years Boss seems to have changed his mind as to the reality of this term; for in his Preliminary General Catalogue, published in 1910, he treats Algol as though its motion were uniform, although in the

case of other stars in this catalogue he devotes much attention to periodic inequalities.

It should be remarked that the absence of an appreciable periodic term in the proper motion does not necessarily imply the non-existence of Chandler's third body, since his theory does not demand any particular coefficient for this periodic term. The only condition is that that coefficient must be at least twenty times the star's annual parallax, and thus an accurate determination of the latter quantity would throw some light upon the present question. Unfortunately no determination of the parallax accurate enough for this purpose has as yet been made.

Starting with Chandler's inequality of 173 minutes, Tisserand has attempted an explanation that does not assume the presence of a third body. He shows that if Algol be slightly flattened and if the orbit of the eclipsing satellite be somewhat elliptical, the orbit itself will revolve slowly and uniformly in the same direction as the orbital motion of the satellite. Consequently the eclipses will occur earlier than the average time if the periastron point is in the half of the orbit that precedes eclipse, and later than the average if the periastron point is in the half that follows eclipse. This explanation is beautifully simple, and for a time seemed to be the key to the puzzle. I am able to say, not without some regret, that Tisserand's explanation is no longer tenable. In his memoir the following relation is established:

Period $\times$ eccentricity $=$ 3.1416 $\times$ the inequality. In this case the period is 2.87 days, and the inequality found by Chandler is 173 minutes; an eccentricity of 0.13 is therefore demanded, but this is out of the question. A long series of spectrographic observations made at the Allegheny Observatory shows conclusively that the eccentricity of this orbit can not possibly be as great as 0.13, that it is more likely than not to be under one fifth this amount, and that therefore no inequality greater than forty minutes can be plausibly accounted for in this way.

Shortly after Chandler's formula for the inequality was published, the star (always El Ghoul) thereafter began departing from it little by little, until now the eclipses occur more than an hour later than the formula implies. The character of the inequality is once more in doubt, but as the existence of some kind of inequality is beyond question, this does not lessen the necessity for an explanation.

While the chances in favor of the reality of Chandler's third body have been growing less and less, evidence has been steadily accumulating in favor of an entirely different third body in this system. Since the publication in 1890 of Vogel's classic observations, it has been well known that the radial velocity of Algol is affected by an oscillation whose semi-amplitude is about forty kilometers, and whose period is the same as that of the light changes, 2.87 days. In 1906 Belopolsky of Pulkova detected the presence of another oscillation in the radial velocity, the amplitude being much smaller than the other, and the period several hundred times as long. Observations made at the Allegheny Observatory have confirmed this discovery in an unmistakable way. The period of this new oscillation is found to be a little less than two years. It could be explained by the presence of a third body of such mass and so situated that the projected distance from Algol to the center of gravity of all three bodies is about two thirds of the distance from the earth to the sun. It is natural to inquire whether other explanations are not possible, or, in other words, whether the shifts in the spectrum lines from which this third body is inferred

may not arise from other causes than changes in velocity. This disturbing question is one that frequently recurs to the mind of the astronomer. Happily, in this case it can be answered in the negative without hesitation. The presence of the third body necessitates a light equation similar to that imagined by Chandler, but now of course with a period of less than two years and with a small amplitude. This amplitude can be computed in advance; we find that it amounts to about five minutes of time. I have examined the rich photometric material on this star accumulated in the second half of the nineteenth century and have found that this light equation is actually present. This seems to leave no doubt that the shift in the spectrum lines is nothing other than an effect of velocity and that the system of Algol contains at least three bodies, only one of which is visible in even our most powerful telescopes.

It is at this point that the man at the telescope must turn to the mathematician and ask him whether this third body can in any way produce the long inequality in Algol's period, that is, in the time that elapses between successive eclipses. If this should be found not to be the case, what dynamical explanations are possible other than those already tested and rejected?

The answer to these questions would doubtless apply to other eclipsing variables, for many of these show similar inequalities in their periods, though as yet in only one other case has the presence of a third body been demonstrated.

A somewhat similar problem is presented by the so-called secondary oscillations that have been announced for certain spectroscopic binaries. If we observe the velocities in a system as carefully as we can, we may draw a curve that expresses the relation between time and velocity. Curves of this sort from various stars will differ widely from each other, but all must conform to certain restrictions, which are in fact those that follow from Kepler's laws. Now for the majority of binaries this is found to be the case, and by assuming that the orbit of the body we have observed has certain dimensions, shape and situation, the velocity curve can be represented within the limits that the accuracy of the observations leads us to expect. But this is not always so: a number of spectroscopic binaries were found for which the velocity curve did not conform to simple elliptic motion. It was then assumed that the system must contain a third body whose attraction causes perturbations in the place and in the velocity of the bright component that we observe. By adopting suitable mass and distance for this body it was found possible to represent the velocity curve fairly well. Too much emphasis should not be placed upon such a representation, however; the assumption of a third body is very much like the adoption of additional pairs of Fourier terms in an empirical formula, and it would have to be a velocity curve of very complex form that did not resemble, within plausible limits, one of the great variety of curves that so many terms would yield.

It has developed recently that many of the cases in which secondary oscillations were apparently present could be explained as a systematic error of observation. This is caused by the presence on the plates of the spectrum of the fainter component which sometimes blends with that of the brighter in such a way as to distort the measures. Leaving out of account all the stars whose secondary oscillations can be explained in this way, we find that practically all the remaining cases are also variable in their light, but

not in such a way as to permit the eclipse explanation to apply. This circumstance causes the observer once more to inquire whether the shifts in the spectrum lines that he observes are always velocity effects, or at any rate whether they are due to orbital motion. These remaining cases have another peculiarity; the period of the secondary oscillation is always found to be either just one half or just one third of that of the principal oscillation. If we interpret this in terms of a third body we have a system in which the three components are close together and revolve around each other in simply commensurate periods. It is for the mathematician to say whether such a system can be stable, and therefore whether such a third body is possible. Although this is a problem of many years' standing it has not yet been approached from the mathematical side, so far as I am aware. It seems probable to the speaker that such a system will be found to be unstable, for reasons similar to those that account for the dark divisions in Saturn's rings and for the gaps in the distances of asteroids from the sun, these divisions and gaps corresponding to places where the periods would be simply commensurate to that of one of Saturn's satellites in the one case, and to that of Jupiter in the other. It is worthy of remark that in not a single instance where a third body has been inferred from a commensurate secondary oscillation, has this body been confirmed by a subsequent detection of its spectrum or otherwise. It is true that in Lambda Tauri two oscillations, both of short period, have been detected; but these periods seem to bear no relation to each other.

A mathematical problem connected with binaries, more important than either of the above, has to do with the origin of these systems. This is one of the few problems in sidereal astronomy with which the mathematician has concerned himself to any great extent, but it is still far from being in a satisfactory state. The past history of the moon, in a dynamical sense, formed the subject of an exceedingly laborious investigation by George Darwin more than thirty years ago. He concluded that the earth and the moon had once formed a single body and that they had broken away from each other by a kind of fission induced by the rotation of the body on its axis. Tidal friction is now set up; it causes the two bodies to draw away from each other, the month to become longer and the orbit of the moon to become somewhat eccentric. Darwin and others have extended this reasoning to double stars, and here the recent work on spectroscopic binaries seemed to afford a striking confirmation of the theory. It has been found that close binaries almost invariably have circular orbits and that their physical condition, as revealed by their spectra, is of the sort that is generally accepted as indicating youth. Widely separated binaries, on the other hand, are apt to have eccentric orbits and to show signs of old age. Still more recently the mathematical side of the question has been reviewed by Moulton, Jeans, Russell and others. It now appears that Darwin's results are at least incomplete and that the causes he adduces are not sufficient to account for the genesis of the moon or for that of double stars. The chief difficulty is that tidal friction is not competent to drive apart to any great distance two bodies of comparable mass that have separated by fission. It appears probable in this view that the separation must have occurred long before the bodies formed stars, that is, while they were still nebulæ. The difficulties of reconciling certain observational facts with this view are

great, but it would be out of place to recount them here.

We see that binary systems offer a rich field for the labors of the mathematician. Other subjects in astronomy are equally inviting, and I have no doubt that other sciences have as much to offer. An eminent psychologist, for example, has said that the time has come for a great mathematician to concern himself with psychological problems. There is a proverb to the effect that to him that is well shod the whole earth is covered with leather. And so the mathematician may walk where he pleases. What particular path he chooses is not a matter of great importance, but it is important that he be abroad and doing, and that he do not sit at home admiring his shoes.

Science has often been likened to a warfare, and such a simile as this naturally recurs to the mind at this time. We may think of science as at first occupying a small domain surrounded by the vast territories of the unknown. In the early days it was easier than now to add to this domain. A single bold spirit, starting out in almost any direction, could often wrest much from the adversary. But as the domain of science increases, so also do the extent and diversity of its boundaries. The more obvious points of vantage are already taken and the character of the warfare must change. The day of guerilla warfare is gone, it is now necessary to act in larger groups and for each man to be willing to serve at the side of others. This policy often requires the suppression of personal ambition, and deeds of individual heroism become less frequent; but great victories are to be won in either kind of warfare only if the soldier is imbued with such a spirit as this.

FRANK SCHLESINGER

ALLEGHENY OBSERVATORY OF THE
UNIVERSITY OF PITTSBURGH

THE PLACE OF FORESTRY AMONG NATURAL SCIENCES[1]

IN an old forest magazine, *Sylvan*, is a story about Germany's great poet, Karl von Schiller. Schiller, taking rest at Illmenau, Thuringen, met by chance a forester who was preparing a plan of management for the Illmenau forest. A map of the forest was spread out on which the cuttings for the next 220 years were projected and noted with their year number. By its side lay the plan of an ideal coniferous forest which was to have materialized in the year 2050. Attentively and quietly the poet contemplated the telling means of forest organization, and especially the plans for far distant years. He quickly realized, after a short explanation, the object of the work and gave vent to his astonishment: "I had considered you foresters a very common people who did little else than cut down trees and kill game, but you are far from that. You work unknown, unrecompensed, free from the tyranny of egotism, and the fruit of your quiet work ripens for a late posterity. Hero and poet attain vain glory; I would like to be a forester."

An opinion not unlike that held by Schiller before meeting with the forester still commonly prevails in scientific circles in this country. It is quite generally believed that foresters are pure empiricists; something on the order of gardeners who plant trees, of range-riders who fight forest fires, or lumbermen who cruise timber, carry on logging operations or manufacture lumber and other forest products; that for whatever little knowledge of a scientific character the forester may need in his work, he depends on experts in other branches of science; on the botanists for the taxonomy of the trees, on physicists, chemists, and engineers for the proper understanding of the physical, chemical

[1] Paper delivered before the Washington Academy of Sciences on December 3, 1914.